38287/P
63189

# A REPLY

TO

## A LETTER OF MR. OWEN;

BEING A DEFENCE

OF THE

## ORIGINALITY OF MY DISCOVERIES

IN

## DENTAL DEVELOPMENT.

---

BY

ALEXANDER NASMYTH, M.R.C.S., &c. &c.

---

[*From* THE LANCET *of July* 11, 1840.]

LONDON: PRINTED BY W. TAYLOR, 49, ESSEX STREET, STRAND.

1840.

# MR. NASMYTH'S REPLY TO MR. OWEN.

[*From* THE LANCET *of July* 11, 1840.]

*To the Editor of* THE LANCET.

SIR:—I lose no time in undertaking the "explanation" and "justification" of my views on dental development, but before entering upon that duty, allow me to make an observation with respect to the separate publication of the abstract of my papers, read at the British Association, and contained in the forthcoming volume of last year's "Transactions." Had I been aware that such a cunning outcry would be raised on what I shall proceed to show is altogether a groundless assumption, I, certainly, for my own sake, should not have sent you that abstract for review. It was the most convenient and concise, as well as the only corrected document for me to refer to on the subject; but my opponent has reaped far more advantage from it than I have, by attempting to raise a prejudice against me with respect to it. That that prejudice is utterly unfounded, my conduct will at once show; for so far from being forced to rely solely upon this abstract in "justifying" and "explaining" my views of dental development, I have really no occasion to refer to it at all, and shall proceed to my task with, simply, the reports published last year in the "Literary Gazette" and "Athenæum," before me. I may remark, however, that I had copies of the abstract for some time in my possession before I sent one to you, and that I had received repeated assurances from Mr. Phillips, in writing, that I was at liberty to give publicity to my views on the subject of dental development in any way I thought proper.

My motive for sending you a copy of it for review, was very natural; I wished you to compare my own account of my researches with Mr. Owen's history of his "Nouvelle Theorie," in order that you might not have to complain, that you were only made acquainted with my views, through the report of a third party. The report of my papers, given in the "Literary Gazette," was not my own, whilst that which I sent to the "Athenæum" was so altered and abridged, that I cannot be considered responsible for it. However, I have not the slightest reluctance to challenge Mr. Owen to a comparison of our respective pretensions, even under the disadvantage of quoting from hasty reports of my own papers, made or modified by other persons; whilst he is at liberty to appeal, in his defence, to a statement, carefully drawn up by himself, after my views had been laid before two different sections of the British Association (of which he is one of the Council, and was present at the meeting), and after the publication of the reports of my papers. With respect to my "Abstract," I at once state that it contains no interpolation whatever of matter not contained in the "Literary Gazette" and "Athenæum," and I am therefore quite at a loss to divine why I am not justified in having sent it to you for review. I conceive, on the contrary, that I was only doing bare justice to myself by taking this step.*

* As Mr. Owen has published in another journal, as well as in THE LANCET, a long and rambling letter to Professor Phillips, charging me with introducing matter into my "Abstracts" of the papers read at the British Association, which was not contained in those papers themselves, allow me to extract here, my reply to his accusation:—

"*The abstract of my papers given in the 'Transactions' of the Association contains no interpolations whatever; no matter which was not contained in the papers themselves:* I wish the same could be said of all Mr. Owen's reports published in the same work. The promptness with which that gentleman at once accuses others of the practice of interpolation, appears to have its origin in the circumstance, that he is far from being unfamiliar with that practice himself. I should advise him, before he again 'prays' Prof. Phillips to compare my abstract for the 'Transactions' with the reports in the 'Literary Gazette' and 'Athenæum,' to request the secretary to institute a similar comparison between his own abstract, in the 'Transactions' published in 1839, and the reports given in the weekly journals, shortly after the meeting of the Association in 1838. Prof. Phillips would, by so doing, be able to make out a

Indeed, a more absurd attack than Mr. Owen's accusation against me can scarcely be conceived. A paper is read by me in August, 1839, and because I dare to distribute an abstract of it *so early* as June, 1840, I am to be the object of a tirade from an individual who, between the reading of my paper and the publication of the report of the

---

case of interpolation so flagrant, that all Mr. Owen's disinterested indignation against such proceedings would be perfectly explained. In the eighth volume of the 'Reports' of the British Association for the year 1838, p. 136, the following passage occurs in the abstract of Mr. Owen's paper:—

"'Purkinje and Fränkel also added to dental anatomy several new and interesting facts relating to the structure of the enamel, pointing out, more especially, the form and characteristic transverse striæ of the component crystals: and lastly, they determined the true osseous nature of that distinct layer of substance which had been previously known to surround the fang in the teeth of man, and which they once observed to be continued on the enamel of a human incisor. *This observation Mr. Owen proceeded to state he had confirmed, and he exhibited several sections of the simple teeth of the mammalia, in which both the ivory and enamel were invested by a layer of osseous substance, identical in its structure with the cement which enters more abundantly into the composition of the compound teeth of the herbivora.*'

"Not a word, however, of all this is contained in the lengthy report of Mr. Owen's paper printed in the 'Athenæum,' which *he* acknowledged to me was furnished by *himself*, nor in the 'Literary Gazette;' and for the best of all reasons, viz., that at the time when his paper was read, and the 'Athenæum' report of it published, Mr. Owen had *never seen* the work of Purkinje and Fränkel; though in his report, he would fain lead the reader to believe, that he had been well acquainted with these writers in the autumn of 1838. The following letter to me shows, that he did not even receive a copy of their work till the spring of 1839:—

"'April 1, 1839.

"'My dear Nasmyth,—I received from Baillière, on Saturday last, the inclosed copy of Fränkel,* and as I shall not have time to look into it till the end of the lectures, I send it to you, knowing your anxiety to see it. I may ask for it again towards the end of June. * * Believe me, dear Nasmyth, yours very truly,

"'R. Owen.'

"After perusing this, I think you will agree that Mr. Owen is not exactly the person, in whom it is safe policy to write a letter to Mr. Phillips, containing unjust accusations of interpolation against another party. Mere interpolation, however, although not to be commended, is a matter of

---

* In which Purkinje's researches are published.

---

no great consequence, unless it be adopted from some reprehensible motive. Let me state what was the motive of Mr. Owen in this particular instance. It can have been no other than that of appropriating to himself a discovery, to which he had not the slightest claim, as I shall prove even by his own written admission. By his own report of his paper, read at the British Association in August, 1838, we learn, that he then made no mention of the enamel capsule, nor did he of the researches of Purkinje and Fränkel, for the very valid reason communicated above. Shortly after his return from Newcastle and the Continent, however, I sent him a paper containing an account of my discovery of the enamel capsule, which was afterwards published in the twenty-second vol. of the 'Med.-Chirurgical Transactions.' This paper he returned to me with the following note, dated

"'December 11, 1838.

"'Dear Nasmyth,—Hearing that you had made inquiries about the MSS., I return them without delay. I have marked in pencil all that occurred to me in the way of amendment, and I think it will be an acceptable paper for the Medico-Chirurgical Society. I am, in haste, faithfully yours,

"'R. Owen.'

"In April, 1839, he obtains the work of Purkinje and Fränkel, and then, on preparing the abstract of his papers read at the Association in the month of August previous, he inserts a passage, in which he represents himself as having there stated that he had confirmed and followed out their researches, and anticipated my discovery of the existence of a capsular covering external to the enamel.

"It is a great pity, as I have before hinted, that Mr. Owen, who, in the year 1840, displays such virtuous zeal, in writing to Mr. Phillips to prevent the reports of papers for the 'Transactions' from being modified, so as to comprise an account of discoveries made since those papers were read, did not manifest the same laudable anxiety in the year 1839, since, at present, his ingenuous cries for justice are quite uncalled for; whereas, last year, had he prayed Professor Phillips to compare the proofs of abstracts for the 'Transactions' with the reports published at the time, in the hebdomadal journals, I might have been spared the pain of exposing what, I think, must appear to all impartial eyes a very equivocal transaction, more especially as it was perpetrated under the guise of friendship."—*Med. Gaz.*, No. 40.

Association, has been writing on the same subject in analogous terms in two countries, and, if I mistake not, in more than two places.

One other introductory observation, Mr. Editor, you must also permit me to make, in reply to an impertinent insinuation of Mr. Owen. He would, it seems, have his readers believe, that I am the author of the review which appeared in your Number for June 6th. Now, I beg to state, and you will bear me out in the assertion, that the article in question was not written by me, and that I did not see a line of it before it was published.

In answer to the first charge brought against him, that he had incorrectly stated recent writers to have regarded the teeth as inorganic, Mr. Owen replies, that he used the word "inorganic" in its *German* sense. This is, doubtless, a very subtle answer, but it labours under the disadvantage of being very little to the point; for in my papers, as reported in the "Literary Gazette" and "Athenæum," I described the teeth (as I shall shortly show) to be formed by a process of ossific "transition," and, consequently, as not being inorganic in any sense of the word, German, French, or English. I, therefore, maintain that Mr. Owen's assertion, that recent writers, without even excepting me, had regarded the teeth as inorganic, is decidedly incorrect.

Men are generally most violent where they are most in error: at any rate, no where in Mr. Owen's letter is he more abusive than where, labouring under a mistake himself, he tries to fix a charge of plagiarism upon me. He accuses me of gross injustice to recent writers, and to Retzius more particularly, in claiming the discovery of the cellular structure of the interfibrous substance. Now the fact is, the researches of Retzius, which he is pleased to assume I have appropriated, only go to establish the existence in the ivory of osseous corpuscles, principally towards its periphery, on which, he states, the tubes ramify, and in which some of them terminate. The interfibrous spaces he does not expressly describe, further than by stating that ramifications of the tubes course across them. Of what is further contained in them he has actually given no account.

In support of his altogether unfounded and unaccountable assertion, Mr. Owen, in order, apparently, to impose on the gentle reader, parades a reference to Baly's translation of Müller, Part I., p. 427, 2nd edition; but if the reader, in conformity with his recommendation, should consult this same translation, he will there find, that Dr. Baly, strange to say, in direct opposition to the inference Mr. O. wishes his readers to draw from Part I., p. 427, 2nd edition, *has not* fallen into the error Mr. Owen has; but, on the contrary, after noticing the researches of Retzius, adds these words:—"Mr. Nasmyth is represented as stating, that the intertubular substance of the ivory is *not structureless*, but distinctly cellular!"

How, therefore, can I be an "unblushing plunderer" of Retzius, in describing these spaces as being filled with ossific substance of a cellular structure, which he has scarcely mentioned, except to mark their extent? Purkinje, whom I am also accused of injuring, by announcing as my own the discovery of the cellular character of the interfibrous substance of the teeth, actually believes that "close as the fibres are together, the *structureless* (*structurlose*) intermediate substance still forms the greatest part of the mass of the tooth."* This passage is extracted from Müller's review of the progress of anatomy and physiology in the year 1835; and as he expresses no dissent from the statement it conveys, the natural inference is, that he agrees with it. In what way, then, have I been unjust to, or how have I plundered from Purkinje and Müller, by announcing as my discovery the cellularity of the interfibrous substance? Mr. Owen states that Müller discovered the fibres of the ivory to be tubes; this is not correct; Müller himself attributes the discovery to Purkinje. It were really to be wished that Mr. Owen would study a question before he takes upon himself to write on it dogmatically, and that he would be still more careful not to make ignorant accusations which can only redound to his own disgrace.

With respect to the pulp, I claimed in my papers no discovery in regard to its internal organisation, though I think my researches were more minute than those of previous anatomists. I stated, in August, that it was mainly composed of "a number of minute cells in a vesicular form," and invested by a "reticular" membrane. On the 16th of December, Mr. Owen states, in his history of a "Nouvelle Theorie" of the formation of the teeth, that it consists of "semi-opaque polyhedral granules, or cells suspended in a clear matrix, and the whole inclosed in a tough transparent membrane, which forms the outer surface of the pulp."—*Comptes Rendus*, p. 786. Both these descriptions were made with a view to facilitate the demonstration of the process of dentification. I leave the analogy between them to be judged of by your readers.

Mr. Owen claims the discovery that the teeth are not "bodies of an inorganic nature, formed like brute bodies, by the juxta position of layers successively exuded by a glandular pulp or membrane," which, he says, is the theory of previous writers,

* See Müller's Archiv for 1835, P. II., and also a notice of the passage quoted above in my "Researches on the Teeth," published last year, p. 46.

*myself included;* but that they are formed by the transition of the pulp itself into ivory, by a "centripetal" deposition of ossific matter in its cells. He tries to make out that I am in favour of the former doctrine, and can, consequently, lay no claim to his "Nouvelle Theorie," by omitting, in his reply, all the passages in the reports of my papers which are adverse to his purpose, and by ingeniously selecting all expressions of the reporter, which, when isolated, seem or can be made to bear a meaning favourable to his object. Now let me show, by extracts, what my papers really do say: for what they do *not* say, Mr. Owen's account may be very advantageously consulted. First, and foremost, let me observe that I, according to Mr. Owen, a supporter of the old theory of exudation, in opposition to that of transition, was at the unaccountable pains, last summer, of causing to be made a great number of large diagrams *expressly for the purpose of showing the cells of the pulp in their ossific transition into the cells of the ivory.*

The original sketches of these diagrams and drawings had been previously, for a considerable period of time, accumulating under my hands, as specimens occurred in my researches from which they could be made. When the collection had been completed by my excellent and talented artist, Mrs. Holmes (who, as well as several other persons, can prove, without the slightest difficulty, that it was in existence some time before the meeting of the British Association), I took it with me to Birmingham, and there exhibited it in illustration of my papers. Had I been in favour of the old doctrine of dental formation, I should scarcely have written a long paper on the subject, and should certainly not have gone to the trouble and expense of having a large collection of illustrations made, in confirmation of previous views. Moreover, as will be seen by referring to the "Lit. Gaz.," I stated that my theory was both "bold and novel," though Mr. Owen ridiculously endeavours to show, that my descriptions differ in no respect from those of previous writers. The diagrams above mentioned I have since exhibited at a lecture delivered by me at the Royal Institution; and they are all, as well as the drawings, still in my possession.

In order to examine the pulp for the purpose of studying the process of dentification, the ivory already formed at the surface of the pulp (for the transition goes on gradually from the circumference towards the centre) must be forcibly separated from the unossified portion beneath. When this has been done, the question suggests itself, whether the ivory already formed, fragments of which have, of course, in the disruption, been left strewn on the surface of the pulp, has been secreted or exuded by the pulp; or is, on the contrary, an ossific transformation of the latter. I have shown that I am altogether in favour of its being an *ossific transformation;* the words "excretion" or "exudation" never occur in my paper, any more than the corresponding ideas did to my mind; on the contrary, I said that the cellular fragments found upon the surface of the pulp "*are in size and appearance perfectly accordant with the cellules of the pulp;*" the only object of which observation was, of course, to prove that the latter are converted into the former; and I went on to state that, at an early stage of development, "the different layers of cells will be seen, and *the transition into ivory observed.*"—*Lit. Gaz.* for Sept. 21, 1839, p. 598. Here I exhibited no fewer than 50 drawings before the medical section, illustrating this process of transition. In describing the process of ossific transition in the very cells thus depicted, I am represented by the reporter as having stated, that "when merely a thin layer of ossific matter has been deposited on the surface of the pulp, it may with great facility be drawn out entire." Mr. Owen fixes triumphantly on this passage to prove that I am in favour of the old theory of exudation, though this observation was made of a drawing to which I was actually pointing at the time, to show the appearance *of the transition of the cells to an ossified state.* The fact is, when the external layer of the pulp becomes ossified, it can no longer be regarded as the pulp. It is then spoken of as a layer of ivory in apposition to, and in connection with, the surface of the pulp beneath. Thus the deposition of ossific matter on the surface of the pulp simply means its deposition in the cells of the reticular, formative surface, which is undergoing the process of dentification. That the idea of exudation could not even have been floating before my mind, is proved by a subsequent passage, where I state, "*that the manner in which the osseous matter is deposited in the cells of the interfibrous substance, I had not been able to discover.*" It argues no little assurance, to say the least, on the part of Mr. Owen, to assert, after quoting this sentence of the date of Sept. 21, which he does, that I was ignorant of, and, indeed, opposed to, the theory of the formation of the tooth by ossific deposition in the pulp, when I read my paper; that I altogether changed my views on the appearance of his memoir of the 16th December, and that I then borrowed it from him. He surely would not venture to make such outrageous assertions, if he did not calculate on his position and influence for carrying him through any controversy with impunity, however unfairly it may be conducted on his part.

In the "Athenæum," No. 620, p. 707, I said, that the "cellules of the fragments of the ivory which are found scattered on the pulp, *resemble exactly in size and appearance the cellules of the latter, when in a state of transition.*" It is quite futile for Mr. Owen

to argue in the face of these passages, that I am in favour of the old theory of exudation, simply because now and then I speak of the ossified portion of the pulp as a layer of ivory in apposition to the formative or transitive surface of the yet unossified pulp beneath. With equal, if not more, reason, I might infer that he is a supporter of the old doctrine, from a passage of his, at p. 504 of the "Medical Gazette," where he speaks of the "well-known facility with which the layers of ossific matter can be detached" from "the surface of the pulp." I, also, in opposition to former writers, hold, that the layers of ivory are not merely in apposition to, but are also in organic connection with, the pulp; not only are all my observations on the process of transition in favour of that view, but the fact that, after the forcible separation of the ivory from the yet unossified pulp, cellular osseous fragments are found strewn over the latter, showing that they were organically connected before the disjunction, positively confirms it. It does not suit, however, Mr. Owen to admit this, and so he roundly asserts that I regard these cells as "excreted," although neither that nor any similar word occurs in the lengthy report of my papers. So much for his fairness and conscientiousness! He perverts my meaning where he can; and when perversion fails to effect his object, he instantly draws upon his imagination, and attributes to me the first phrase that occurs to him, which it would suit his case for me to have uttered. Besides, who ever heard of the exudation or excretion of ossified cells? the idea of such a physiological process is an absurdity; and if his case were not desperate, you may rest assured that Mr. Owen would find a better means of escape from the charge brought against him, than by attributing to me such inconceivable nonsense.

Mr. Owen's defence of the originality of his discovery of the correspondence between the granules, or cells of the pulp, and the fibres of the ivory, appears to me to be very lame. I had clearly stated the identity of the dental fibres with the fibres of the surface of the pulp; but he tries to represent me as a supporter of the old theory, because the reporter of the "Literary Gazette" uses a passive instead of an active verb, in describing my observations. His interpretation is so laboured, as to be a curiosity in the annals of sophistry. Finding in the "Literary Gazette" the phrase, "that the framework of the reticulations or cellules of the pulp is constituted by the fibres of the tooth;" although it is evident, that to express properly the meaning of the phrase, "constitutes" should have been used instead of "is constituted by," he does not hesitate to assert, from this inaccuracy of the reporter, in opposition to the whole tenor of the papers, my meaning to be, that the fibres of the teeth, after having been excreted or exuded by the pulp, produce fresh fibres of the same diameter and similarly curved, on the surface of the latter; he would fain make your readers believe, that I have described the fibres of the reticular surface of the pulp as being merely the impression from the fibres of the superimposed ivory; though in the "Athenæum" (No. 620, p. 707) I have expressly said that, "from the spirally fibrous framework of the reticulations are evolved the spiral fibres of the tooth;" by which evolution it would be ridiculous to suppose that I meant any process resembling "excretion" or "exudation."—I merely wished to state, that at the formative or reticular surface of the pulp, where, to use my own words, "the osseous matter is deposited in the cells of the interfibrous substance," the fibres themselves, by a process of evolution or development, are converted into the fibres of the ivory. I do not deny that Mr. Owen's descriptions may be clearer than those ascribed to me, for he drew them up himself, at his leisure, whereas my reports are the hasty productions of a third party; but if the reports in the weekly journals are to be thus unfairly dealt with, men who cannot make science their sole and exclusive occupation, had better retire at once from the British Association, and leave to figure there those gentlemen only, who, like Mr. Owen, have time to publish their "nouvelles theories" whenever they please, and who do not scruple to interpret just as it suits them, and to take advantage of hasty reports.

You alluded in your article to the analogy which existed between that portion of Mr. Owen's memoir, in which he says that he had been unable to recognise the "precise arrangement of the hardening salts" in "the external membrane of the pulp;" and the following passage in the report of my paper, given in the "Literary Gazette:"—"The manner in which the osseous matter is deposited in the cells of the interfibrous substance, he (Mr. N.) had not been able to discover." In his reply, Mr. Owen says that his observation refers to the external membrane of the pulp, whereas mine relates to the interfibrous cells. This is mere shuffling; for the interfibrous cells to which I allude, are those of the external surface of the pulp, inasmuch as it is there alone that the process of ossification is carried on. His other observation on this subject is a petty quibble. It is not, forsooth, the "manner" in which the osseous matter is deposited that has escaped him, but it is the "arrangement" of the same, when deposited, that he has not been able to "recognise;" as if the one did not imply the other. Had he "recognised" the manner of deposition, he would have been acquainted with the arrangement; and if I had been able to discover the arrangement, I should have obtained a clue to the manner of deposition

—we were speaking, therefore, in reality, of the same thing. Paltry as is Mr. Owen's defence, he assumes quite a triumphant air after having made it, and thinks himself privileged by his success to storm at you, Mr. Editor, and to throw out an emphatic insinuation against me, which is as false as an "emphatic assertion" represented to be mine, which, in another place, he has pretended to quote from my report, *but which is in reality, as has there been shown, his own fabrication.**

Certainly, he is the last person from whom one would expect to hear a boast, which is meant to reflect discourteously on the character of his adversary,—that *he* possesses friends "who are the very antipodes of impudence and duplicity." The matter can only be explained by supposing, that his ideas of what constitute modesty and candour are the very antipodes of those generally received in this quarter of the globe.

---

* "At p. 507 of the last Number of this Journal, he (Mr. Owen) manufactures what he is pleased to call Mr. Nasmyth's 'emphatic statement' of September, 1839, 'that so far from being the ossified pulp, it (the dental substance) was altogether a distinct formation.' In making this fabrication, he appears to have been assisted by a friend; and, in order to give it greater weight, he takes the trouble to inform us that the character of that friend 'is the very antipodes of impudence and duplicity.' Now of this passage, which he gives as quoted from the 'Lit. Gazette,' p. 598, only *the last four words will be found*, on reference to that journal, to be correctly copied: *all the first part of the passage is the composition of Mr. Owen.* Mr. Nasmyth, by this perversion of his report, is made to say that the dental substance is altogether a distinct formation, whereas he never mentions the dental substance at all; and his real meaning evidently is, as we have shown above, that the cells of the ivory have undergone a distinct formative process in their transition from the cells of the pulp."—*Med. Gaz.* No. 43, p. 596.

The passage which Mr. Owen has thus misquoted, and upon which he principally relies for making out his case against me, occurs in the report of the "Lit. Gazette," *after I have finished the exposition of my own views*, and where I am speaking of those of Schwann. This author regards the pulp as simple cartilage, and as being directly converted into bone; whereas my researches tend to show that, by a distinct formative process carried on at the surface, the vesicles of the pulp are converted into cellules, previous to the deposition within them of osseous matter; and hence the reporter for the "Literary Gazette" was quite justified in stating, that "Mr. Nasmyth regarded the cells of the ivory as a distinct formation."

In the reply to the charge which you very justly brought against Mr. Owen, that he exported beyond seas, and first published in a foreign language, his so called "*Nouvelle Theorie*," he asserts that the theory of dental development, propounded and supported by the researches described in his memoir, were first dutifully submitted to the members of the Royal College of Surgeons, in his lectures on the teeth, delivered in May, 1839. You will please to observe, Mr. Editor, that Mr. Owen, who has attempted to excite a very gratuitous opposition to my claims, as represented by an "*Abstract*," which has been printed and in my possession for two months, because the volume in which it is contained, and which Mr. O. with marvellous assurance denominates a "nonentity," is not yet out, here appeals, in his defence, to a series of lectures *which have never been printed or published at all*, and which, perhaps, only exist in rough manuscript notes. Pray observe, also, that though I, myself, am not to be allowed to give any evidence, or be any authority on the theory propounded by me at the British Association, but am to abide patiently and implicitly by the account of it furnished by the reporters or editors of the "Literary Gazette" and "Athenæum," Mr. Owen's word is, it seems, to be taken at once, as to what he did or did not state in his oral dissertations of 1839. The injustice here done to me I need not point out; it is perfectly accordant with the line of conduct which Mr. Owen has for some time past thought proper to adopt with regard to me; and it is only one of the many proofs I have received, that in the discussion of this question by Mr. Owen, he will not be at all scrupulous in regard to what arguments he has recourse to. But, strong in the justice of my cause, I will still maintain my ground, though non-extant lectures and researches taken for granted are appealed to against me; though, by the reputation and influence of my opponent, it will, of course, be sought to extinguish me; though the Secretary of the British Association is already summoned to denounce me; and though, doubtless, the National Institute of France will next be required, in gratitude for the honour done it by the publication of *nouvelles theories* in the "Comptes Rendus," to contribute to the demolition of the daring pretender, by whom the originality of these theories is impertinently disputed. But to return to the immediate question:—Is it at all probable that Mr. Owen would propound in his lectures an important and entirely new theory on the development of the teeth, without recording it in some publication, so as to establish, beyond dispute, his claim to it, and prevent its being appropriated by another? How came it to pass, that this theory, represented to have been thus liberally laid before the scientific world, remained dormant, so to say, for eight months,

and was then given as a "*nouvelle theorie*" to the French Institute? Above all, and I particularly recommend this question to the cogitation of Mr. Owen, and to those who, in conformity with his recommendation, have turned to Dr. Baly's translation of Müller, in their examination of this question,—how is it, that, in the second edition of that work, Dr. Baly did not incorporate the important discoveries stated to have been announced in May, 1839? Mr. Owen has the pleasure of Dr. Baly's acquaintance, which I have not, and would, doubtless, mention such discoveries to him, as he actually did with respect to his researches on the kidney; he also, of course, knew, which I did not, that a second edition of Part I. of Müller's "Physiology" was in preparation, and could not, naturally, be otherwise than anxious that it should not appear without recording the "nouvelle theorie" to which he says he had recently given birth. I have been long unremittingly engaged in researches on the teeth, and had such a theory been clearly announced, or even roughly sketched, either in writing or by graphic delineations, by Mr. Owen, in his lectures of the spring of last year, I could not fail to have heard of it: I did not, however, attend these lectures myself, and, therefore, can furnish no direct evidence on the subject; but immediately after perusing Mr. Owen's reply to your article, I wrote to a friend, who had heard the whole of them, a letter of inquiry, and received from him the following answer:—

"June 29, 1840.

"My dear Sir:—In reply to your inquiry, I beg to inform you, that in the course of lectures delivered at the College of Surgeons in 1839, I do not remember that Prof. Owen made any mention whatever of the new views of the development of the teeth by an ossification of the pulp. I may here state that he did allude to the enamel capsule; in so doing, he mentioned your name, but in a manner that led me and others to believe that you had not discovered the capsule, but that he himself had done so; he added, that the latter discovery was the most interesting that had been made in this branch of anatomy since that of the 'tubular system.' Yours, &c.

"Alex. Nasmyth, Esq." "* * *"

From this evidence, and also from that furnished by another friend, who has written to me on the subject, in equally conclusive terms, as well as for the reasons above detailed, I must be allowed, at present, Mr. Editor, to question the accuracy of Mr. Owen's assertion, that he announced his "nouvelle theorie" of December 16, in his lectures of the previous May.

Mr. Owen has accused me of "versatility," and has complained of the injury which the prosecution of my claims may inflict on the sale of his "Odontography." His accusation I have repelled, and to sympathy on the score of injury I can certainly prove that I have a far juster claim than Mr. Owen. To show your readers with how bad a grace such charges and lamentations come from Mr. Owen, allow me to extract from a letter, published in another place, what I have been compelled to state on this subject. The reader will soon see how coolly my interests have been sacrificed by his versatility.

"Mr. Owen appears to possess certain qualities to such a remarkable extent, that he cannot imagine them absent in others: for instance, he accuses me, throughout his letter, of versatility—a quality with which I have never, to my knowledge, been before reproached, but which I am quite sure, and shall immediately proceed to render evident, he possesses in no trifling degree. In the summer of 1838, I prepared for the press a translation of the work of Professor Retzius, of Stockholm, entitled, "Researches on the Microscopic Structure of the Teeth," and advertised it for publication; this translation Mr. Owen borrowed of me in the course of the same summer. Before advertising its publication, I had inquired of Mr. Owen whether he intended to publish anything on the subject. He replied in the negative. However, as he continued to retain my manuscript of the translation of Retzius in his possession, and as other circumstances had led me to believe that he might, nevertheless, entertain the idea of writing on the structure of the teeth, in which case I should with pleasure, as I told him, have abandoned my intention in his favour, I repeated the question in a letter, to which I received the following reply:—

"'July 25, 1838.

"'Dear Nasmyth:—Many things have interfered to prevent my returning you the translation of Retzius earlier, but as you have not sent for it, I hope without inconvenience to you. *As I have before said, I have neither desire nor object in bringing before the public any of the general observations on the structure of the teeth, which I once hoped were new; but now perceive to be mainly anticipated by the industrious and sharp-sighted Swede.* It will obviously, however, be a source of great credit, and a matter of importance, to whoever practising in the line of dental surgery should combine these discoveries with the *practical* or *remedial* part of the science.† * * * * * * * * * * * * *

"'Believe me, dear Nasmyth,
ever yours,
"'R. Owen.'

"It will scarcely be believed, but it is no less a fact, that, in spite of this letter, and

† The remainder of this letter I omit, as it contains matter which Mr. Owen wished to be considered confidential.

of my having, in consequence of Mr. Owen's repeated assurances, advertised a work on the subject, of which the translation above alluded to was to form a prominent part, that gentleman, within about one calendar month after the date of the above epistle, not only published all the new 'general observations on the structure of the teeth,' but also all the 'practical' deductions from them which he was capable of drawing; that is to say, besides the anatomical details, he laid before the Geological Section the deductions from them bearing on natural history generally, which I had already alluded to in my advertisement; and before the Medical Section, he treated the subject, 'combining these discoveries with the practical or remedial part of the science.' For proof of this, the reader has only to refer to the 'Athenæum' of September 1st, 1838, and to the 'Literary Gazette' of September 15th, 1838, where the 'elaborate and voluminous' reports of his papers, read at the British Association, on the structure of the teeth, are published. What renders this proceeding still more remarkable is, that he gave his 'analysis of the laborious and accurate microscopical observations of Professor Retzius, as related in the original Swedish memoir of that author' (vide 'Athenæum'), a memoir with which he was only acquainted through the translation he had borrowed of me, which he had never asked permission, and had, therefore, no right to make use of, and which he had returned to me, as the reader has seen from the above note, with an assurance 'that he had neither desire nor object in bringing before the public any of the general observations on the structure of the teeth.' I think your readers must allow that I have now established my point, and that Mr. Owen is, indeed, versatile with a vengeance.

"The appearance of his 'voluminous' reports naturally vexed me not a little. I was indignant, that a person, calling himself my friend, should publicly make use, for his own advantage, and without my leave, of a manuscript which I had prepared and advertised for publication; but, nevertheless, I remained silent on the subject, because I hoped that his interference was now, at any rate, at an end, and that from him I had no further competition to expect, as he, indeed, gave me to understand was the case. However, I was disappointed. Having once broken his promise, he was not to be deterred from aggravating the offence. It would be wearying your readers were I to follow him through all the evasive windings of his course, from the time when he had 'neither desire nor object to publish,' to the day when the advertisement of the first part of a bulky work on the subject issued from M. Baillière's shop. Suffice it to say, that, at first, he was 'persuaded to publish'; then he was 'determined to publish,' but only on a limited scale; until, finally, he resolved not only to give to the world the whole subject, and nothing but the whole, but also to render his work 'as generally useful as possible.' Throughout his whole correspondence with me, he has shown himself to be nothing if not versatile—to use no harsher term."—*Medical Gazette*, No. 40, p. 547.

I must now conclude this letter with one observation. To the great surprise of several other persons, as well as of myself, Mr. Owen, in his "Odontography," states, that when he submitted his theory to the Institute, he was ignorant of the researches of Schwann. How this could possibly be, I am at a loss to divine, *for the report of my paper in the "Literary Gazette" contains a full account of these researches*, which I read before the Medical Section, and they are also noticed in the "Athenæum." This account, Mr. Owen shows, satisfactorily, that he has repeatedly seen, long before it was expressly pointed out to him by the gentleman who adopted such a peculiar method of expressing his "aversion to impudence and duplicity."

There is also another proof that Mr. Owen must have been familiar with these researches by Schwann before the publication of his "Memoir," and that is an analogy too close to be accidental, between a passage I had read, translated from that author, which is given in the "Literary Gaz.," and a paragraph in Mr. Owen's memoir, contained in the "Comptes Rendus." Of this analogy your readers shall judge by a comparison of the passages. "Against the theory," says Schwann (vide Report of my paper in the "Literary Gazette," No. 1183, p. 598), "that the dental substance is the ossified portion of the pulp, the facility with which the one is separated from the other has been adduced, and he allowed the force of this objection. Nevertheless, it is at any rate weakened by the circumstance, that a portion of the pulp actually remains attached to the dental substance; and by the fact that in half-ossified ribs, for instance, the cartilage can be easily separated from the ossified portion, and it must be remembered that, in both, the separation must be easy in proportion to the difference between the consistence of the pulp and of the dental bone."

"The argument (says Mr. Owen, "Comptes Rendus," for Dec. 16, p. 788) drawn from the slight mechanical connection which exists between the calcified and non-calcified portions of the pulp in the teeth of the mammifera, in favour of the theory of exudation, and, consequently, of the glandular nature of this bulb, might be advanced with almost as much reason, in order to demonstrate that the primitive cartilage of the sternum secretes or transudes by successive layers the osseous nuclei, which in the embryo can be separated with such facility from the

cavities in which they are formed." I think, Sir, that your readers also, after comparing these passages, will be somewhat inclined to suspect that Mr. Owen must have had, at any rate, a *slight* acquaintance with Schwann's researches before the spring of the present year.

I think, Mr. Editor, I have succeeded in proving, that in my papers, the teeth were represented as organised, and as being formed by an ossific transformation of the pulp; and that, therefore, Mr. Owen's omitting at first to recognise this fact, in order that he might publish the theory based on these views as a new one of his own, was as unjustifiable as his recent attempts to show that these views were borrowed from him, must necessarily be puerile and fruitless. I have also shown, that my observations on the unossified pulp are so similar to his as to have required, at any rate, a notice in the "Comptes Rendus," where he applies all the researches communicated, to the establishment of a new theory of dental formation,—that the identity of the fibres of the ivory with a part of the structure of the pulp was announced by me before it was published by him; and that, with respect to the "deposition" or "arrangement" of the ossific matter in the cells of the pulp, he has but made the same statement that I had previously published. I should be glad to enter still further into detail on this subject, and show the full scope of the papers, drawings, and diagrams, which I submitted to the British Association, and which, of course, are but inadequately reported in the weekly journals of the time; but I feel that this is not the place so to do, nor can I expect, after having trespassed so long on your indulgence, that you should grant me more space.

Indeed for my own sake, I regret much that Mr. Owen's conduct should have forced me into this discussion, which has occupied time and attention that it would have been much more useful and agreeable for me to have devoted to other pursuits.*

I am, Sir,
Your obedient Servant,
ALEX. NASMYTH.

London, July 2, 1840.

---

* The concluding paragraph and the signature of the above letter, together with a few trivial corrections, were sent to the printing office too late in the week to be inserted in THE LANCET.

Printed by W. Taylor, 49, Essex-street, Strand.